

A response to ‘Evidence-based or evidence-biased?’

Dear Editor

Kellogg and Anderst describe my systematic review as “one person’s speculative journey into her belief that non-sexual transmission is not rare”. This is a straw man argument. Fortunately gonococcal infection in prepubertal children is a rare event, by whichever means it has been acquired. In her 2005 update of the 1991 and 1992 “Guidelines for the evaluation of sexual abuse of children” Kellogg writes that a positive culture for *N gonorrhoea* makes ‘the diagnosis of sexual abuse a near medical certainty even in the absence of a positive history’.³ Kellogg advises that a positive finding of gonorrhoeal infection in children should be considered diagnostic of sexual abuse. Prior to conducting this review, I accepted her advice. However, my review demonstrates that while gonococcal infection in children is rare, both sexual and non-sexual means of transmission are possible and both should be considered on a case-by-case basis. As the author of the updated guidelines Dr. Kellogg occupies a highly influential position and I am sorry to hear that she will not consider changing her clinical practice in the face of the evidence presented in my review.

I recently encountered a case of gonococcal vulvo-vaginitis in a 13-month old baby with infected parents who denied sexual abuse but admitted to sharing their bed, bath and towels with their infant. This was a young family of low socio-economic circumstances living in a crowded housing situation. The mother would use her sarong as a nappy on occasions. The child had been removed from the parents on the basis that she must have been sexually abused by either her mother or her father and that transmission could only occur “from infected mucus membrane to mucous membrane”. I sought evidence as to whether it was also possible for this infection to have occurred non-sexually via contaminated hands or fomite transmission.

Hence the aim of the review was to examine the evidence on the possible non-sexual transmission of *N gonorrhoea* in children after the neonatal period. It was not a review of “the accuracy and precision of a diagnostic test” rather it was an exploration of the possible modes of transmission of children diagnosed with this infection. Sexual transmission of this organism is not in doubt. The question is “Is there evidence of non-sexual transmission on occasion?”.

Kellogg and Anderst question why I did not include a randomised controlled trial (RCT) comparing “gonorrhoea rates with children who were sexually abused with children who were not”.⁴ However, this was not an RCT but a case-controlled study of 209 sexually abused girls compared with 108 non-sexually abused girls. Two (<1%) of the sexually

abused girls were positive for gonorrhoea and none of the non-sexually abused girls. This study clearly did not meet the review inclusion criterion of including a case of possible non-sexual transmission. What it does show is that gonorrhoeal infection in children is a rare occurrence. Kellogg and Anderst claim that my review omitted “Higher quality studies addressing non-sexual transmission” but fail to reference these. Since publishing my review I have found one further case study of fomite transmission of gonorrhoea – this was via an inflatable doll.⁵

As I outlined in the methodology section, because this was a review of aetiology rather than intervention, inevitably the level of evidence available was from observational studies rather than RCTs. The papers were graded to address the research question as to whether “some of the contained cases were due to definite, probable or possible non-sexual transmission”. Cases such as epidemics of conjunctival gonorrhoea in outback Australia and Africa and accidental transmissions from infected fluids were graded as definite. I graded the Shore paper to which Kellogg and Anderst refer⁶ as containing probable cases of non-sexual transmission because it included three children diagnosed with gonococcal conjunctivitis. Given the epidemiological data of thousands of young children contracting gonococcal conjunctivitis in the absence of genital infection,^{7–14} it seems probable that young children contracting this infection in the eye acquire it from non-sexual rather than sexual transmission. Contrary to Kellogg and Anderst’s claim, I did not rule out the possibility that the children in the Shore paper could have acquired their conjunctivitis from sexual abuse, which is why I graded them probable rather than definite non-sexual transmission. Similarly, in the Nair paper to which they refer,¹⁵ there were cases of gonorrhoea in children for which the authors were unable to establish the source. Nair et al. include the possibility of non-sexual contact. This paper was therefore graded as containing possible cases of non-sexual transmission.

Clearly I was unable to personally contact authors of papers written in the 1800 s regarding the institutional epidemics. The key authors I contacted had been involved with some of the gonococcal conjunctival epidemics in outback Australia, and with reported isolated cases of likely non-sexual transmission.

Kellogg and Anderst object that in some citations I “erroneously list the authors as ‘anonymous’”. However, the guidelines that Dr. Kellogg herself has updated are listed in Medline under anonymous authorship.^{1,2}

Anonymous. Guidelines for the evaluation of sexual abuse of children. American Academy of Pediatrics Com-

mittee on Child Abuse and Neglect. *Delaware Medical Journal*. 69(8):405–12, 1997 Aug. This does not invalidate them as references.

Kellogg and Anderst take issue with the fact that I was the sole author of the systematic review. However, multiple reviewers is not a requirement, and many Cochrane systematic reviews have a sole author, for example Brocklehurst, P. Antibiotics for gonorrhoea in pregnancy. *Cochrane Pregnancy and Childbirth Group Cochrane Database of Systematic Reviews*. 3, 2007. My paper was peer reviewed by several colleagues prior to submission, and underwent formal peer review before publication in the *Journal of Forensic and Legal Medicine*. I have conducted a number of systematic reviews, both alone and in collaboration.¹⁶ Multiple reviewers, however, are usually required for production of guidelines. I have worked in multi-disciplinary teams critically appraising literature for evidence-based guidelines, for example on interventional pain management (see <http://www.acc.co.nz/for-providers/interventional-pain-management/index.htm?ref=6>). While Dr. Kellogg is the lead author of a guideline,³ she does not appear to have herself conducted a systematic review.

I have read and hold copies of all the English language primary literature in my review. Several non-English papers are referred to in the evidence tables. These are annotated as secondary sources of information and reference is given to the reviews in which they occurred. Contrary to Kellogg and Anderst's claim, it is not "scientifically inexcusable" to refer to foreign language papers if the source is acknowledged.

Kellogg and Anderst point out that the institutional epidemics have not occurred for over 50 years. As I mention in my review, this is due to the advent of antibiotics and improved infection control procedures in the hospitals. When the epidemics were widespread, they were of grave concern to paediatricians internationally, as evidenced by the copious and erudite reports and discussions in the leading journals of the time, such as the *Journal of Infectious Diseases*¹⁷ and *Archives of Pediatrics & Adolescent Medicine*.¹⁸

Kellogg and Anderst express the view that the "gonorrhoea epidemics" in the children's institutions may have not been gonorrhoea at all or may have been the result of widespread sexual abuse. As academics they have access to the papers I have referenced and I seriously urge them to read them. They will see that in many of the reported cases the diagnosis was based both on microscopic examination indicating diplococci negative to Gram's stain, and positive culture using the appropriate media. They will also see that contrary to their beliefs, child sexual abuse was widely acknowledged. The index cases entering the hospitals at the onset of the epidemic were often considered to have acquired their infection venereally. They will also read of the meticulous measures that were taken by these clinicians to isolate cases and to bring the outbreaks under control. This research may have become buried in the archives over time, but it still has much to teach us.

Kellogg and Anderst cannot see how poor local hygiene could be related to transmission of gonorrhoea. Hygiene involves conditions and practices that serve to promote or preserve health. A prospective study of an outbreak of gonococcal conjunctivitis found that children with unwashed faces and hands were of higher risk of contracting the disease.¹¹ In the institutional epidemics outbreaks were brought under control by implementing strict hygiene controls such as isolating infected children, introducing measures such as the use of gloves, avoiding the sharing of instruments such as thermometers between children, abolishing communal bathing and use of disposable items such as nappies. My review was looking at the question as to whether N gonorrhoea can be transmitted by contaminated hands or fomite. The relevance of hygiene would appear self-evident.

Kellogg and Anderst refute the suggestion that prepubertal girls are predisposed to gonococcal vulvo-vaginitis. As paediatricians they may be unaware that adult women may develop gonococcal urethritis or cervicitis but do not get gonococcal vulvo-vaginitis. This latter condition only occurs in the prepubertal girl who has a thin, slightly alkaline vaginal mucosa lacking in oestrogen, acting as an excellent culture medium for N gonorrhoeae.

Dr. Kellogg does not acknowledge the possibility of fomite transmission in her guideline update.³ Her paper does not include a single reference to any of the articles I have reviewed that report non-sexual transmission of gonorrhoea. Her statement that gonorrhoeal infection should be considered diagnostic of sexual abuse is therefore reliant on a very selective sample of the literature.

Kellogg and Anderst have not conducted a dispassionate critique of my review. Their letter contains disparaging emotive language. They appear to have misunderstood the research question. Their strawman argument is based on a misrepresentation of my position which they then proceed to dispute. They erroneously suggest that the question of whether non-sexual transmission of gonorrhoea can occur can be answered by RCTs. They appear entrenched in their belief that gonococcal infection in children is diagnostic of sexual abuse.

There is no doubt that children contract this infection sexually. Its detection should result in an immediate and serious investigation. Sexually abused children require treatment and protection. However, as in any forensic investigation, one must examine the evidence without prejudice. There is overwhelming evidence that children may also contract this infection non-sexually on occasion. If the child has not been sexually abused, removal from its parents, placement in foster care and receiving counselling for sexual abuse may cause irrevocable harm.

According to the Centre for Evidence-Based Medicine "Evidence-based medicine is the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients."¹⁹ The evidence provided in my review will assist practitioners in determining the possible aetiology of gonorrhoeal infections

detected in children and therefore make the best possible decision for care on an individual, case-by-case basis.

Conflict of Interest Statement

None declared.

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